

CROSS-CURRENTS



EAST ASIAN HISTORY AND CULTURE REVIEW

Research in Cambodia, Half a Century Ago

Address to the Thailand, Laos, Cambodia Studies Group at the Association for Asian Studies Annual Conference in Toronto, March 16, 2012

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Before I begin, I want to thank Penny Edwards for making my attendance at these meetings possible. And thanks, too, to Erik Davis for arranging this meeting of the Thailand, Laos, Cambodia Studies Group (TLC). In my dialect, TLC means “tender loving care,” and that’s something this eighty-year-old can do with following the siege of earthquakes we have suffered in Christchurch, New Zealand, over the past eighteen months.

This event has given me the opportunity to return to almost the beginning of my academic career: my doctoral fieldwork in Cambodia fifty years ago. (It was preceded by fieldwork in an Inuit community in the Ungava, Northern Canada; not relevant here.) Rereading my publications from that research has allowed me to relive the excitement of my Cambodian year, living with my wife and child in Phnom Penh apart from a month in Siem Reap, where I could hire a cyclo for ten riels and visit the various ruins of Angkor every afternoon.

Research on overseas Chinese was informed by different paradigms in those days. Bill Skinner was a leading thinker in the field, and Maurice Freedman, my mentor and supervisor, was another. Our issues focused on community social structure and nationalism—many of us were supporters of the national liberation movements in Southeast Asian countries. For most of us, Chinese identity was simply a methodological issue.

Sadly, I haven’t returned to Cambodia since I left in August 1963. I had intended to visit during my first sabbatical in 1970, but the right-wing coup by General Lon Nol put an end to that, since I was known to be a supporter of Prince Norodom Sihanouk, the head of state he deposed. Then, for my next sabbatical, I was planning in 1978 to visit Democratic Kampuchea when we heard the dreadful news that Malcolm Caldwell, editor of the left-wing

Journal of Contemporary Asia, had been murdered in Phnom Penh, so again I set aside any idea of going.

In 1962 to 1963 a war was raging next door in Vietnam, and it colored all our thinking. Canadian friends returning to Phnom Penh from a weekend in Saigon showed us a bullet hole in their car, which put an end to our plans to visit Vietnam. Bernard Fall, that brilliant French-American academic who taught at Howard University and was a vociferously pro-French critic of American strategy and tactics, was just completing a term in Phnom Penh when I arrived in 1962, and we inherited his Citroën Deux Chevaux and his Vietnamese maid. Five years later he died while covering a battle with a platoon of American marines when he “stepped on a landmine.” Recent revelations of CIA dirty tricks lead one to wonder exactly how he did die!

The Geneva Conference that ended the First Indochina War in 1954 established an International Commission for Supervision and Control made up of Canadian, Indian, and Polish commissioners. The Canadian ICC Commission on Rue Dekcho Damdin was our mailing address, and the young Canadian commissioner, Tom Pope, eased our entry into Cambodian society. The ICC’s Douglas DC-4 was the only flight to Vientiane, and a ticket cost over a thousand dollars, so I never did get to Laos either.

The Cold War was still strong in 1962, with the Cuban Missile Crisis in October bringing the world very close to nuclear disaster. It seemed far from Phnom Penh, however, almost like a fantasy. Prince Sihanouk delighted in organizing volleyball tournaments in which his Royal Palace team would play against a team from the diplomatic corps, forcing the Chinese and American ambassadors to talk to each other as teammates at a time when there was no communication between embassies.

The Cold War affected my doctoral program in a rather serious way. When I went to the London School of Economics (LSE) in 1959, my intention was to do research in the New Territories of Hong Kong, where Maurice Freedman was keen for me to study the only traditional Chinese rural social organization remaining (since land reform on both Taiwan and the mainland had transformed the Chinese countryside by then). So I began to learn Cantonese at the School of Oriental and African Studies (SOAS). However, my application for a research visa was rejected by MI5 because of my previous left-wing activities in Canada. Hugh Baker eventually did the research on a lineage village in rural Hong Kong, probably better than I could have done!¹

So it was suggested by Professor Raymond Firth, head of social anthropology at the LSE, that I do my fieldwork in a “New Village” in Malaya, the settlements then being built during the “Emergency” in order to isolate the Chinese from the guerrillas in the jungles. So I began to study Malay at the SOAS, and I met that larger-than-life Turkish Malay, Ungku Aziz, who agreed to be my sponsor. It would have been an interesting study of emergent community organization, but, alas, MI5 once again rejected my research visa application, much to the embarrassment of Professor Firth.

Fortunately, I was able to go to Vancouver as an assistant lecturer in the University of British Columbia’s department of anthropology, where I began a year’s research on the Chinese in B.C. This eventually contributed to a book edited by Professor Edgar Wickberg,² but long before it was finished, my application to do research in Cambodia was accepted, much to my surprise and delight.

So I went to Cambodia by default, you might say, but it turned out to be the best possible outcome. Not only could I spend a year in an exotic culture, but it enabled me to do research in the four languages in which I had functional speech: English, of course; French; Mandarin with young Chinese who had learned it in Chinese schools; and Cantonese with the older Chinese of all five speech groups. In earlier days they had done most of their business with Saigon-Cholon, a mainly Cantonese-speaking city, so Cantonese was their commercial language.

My own interest in the Chinese diaspora came out of my attachment to and love of China, where I was born and spent my childhood. Research in China itself was not possible when I entered my doctoral program, so I did the next best thing and turned my attention to Chinese society outside of China. The same could be said for my brother, Donald Willmott, who studied the Chinese in Semarang, Java,³ and indeed for Bill Skinner, whose research on rural social structure near my hometown, Chengdu, was interrupted by the PLA at the end of 1949.⁴ China’s loss was Java and Thailand’s gain, and I hope someone can say the same for Cambodia!

Looking back, I am somewhat embarrassed by the arrogance of some of my youthful publications, and indeed of the discipline itself at that time: we assumed we could go anywhere and study anything in the interest[s] of “science.” In my review of a book by one of Professor Robert Scalapino’s students (on the political opinions of members of a Chinese community based on a mail-in questionnaire survey), I not only rubbished the book but also questioned how the good professor could have granted a PhD for such shoddy research. And

many of my publications contained smarty-pants footnotes curtly expressing my dissatisfaction with the work of some established scholars.

And so I arrived in Phnom Penh in 1962, just fifty years ago this coming July. I was thirty at the time, an eager, brash young student with a wife and child. Out of that year came my doctoral dissertation, which was published in two books. The first, *The Chinese in Cambodia*, received a review from Professor Lea Williams that began, “Good things come in small packages.”⁵ Gratifying indeed. The second, *The Political Structure of the Chinese Community in Cambodia*, didn’t sell as well with such a boring title, but it was in the LSE Monographs in Social Anthropology (no. 42), which was also gratifying.

I suppose my greatest achievement in Cambodia was single-handedly doubling the Chinese population during my year there! Previous estimates were all in the range of 200 to 250,000 (with the exception of the Catholic Apostolic Visitor, who estimated 900,000 in 1963; perhaps frustrated by the lack of converts among the Buddhist Khmer, the Catholic Visitor wished for a larger population of potential converts). My own estimate, based on complicated calculations and numerous assumptions, was 425,000, or 7.4 percent of the country’s total population. Having doubts about my methodology, I showed it to Bill Skinner (we were on a beach in Bermuda—academic life was tough then!), who said that he was impressed with my methods and that they stood up well against other demographic studies in the Chinese diaspora.

When I returned to London, my luggage included two large boxes of IBM punch cards from the 1961 census, one for central Phnom Penh and one for the total population of Siem Reap. They were cards about the size of a letter envelope with numerous holes punched in them, and I dutifully fed them through the LSE’s huge mechanical sorter. That was how we did demographic research before the age of computers.

Some years earlier, Maurice Freedman had published an important comparative paper in which he pointed out that, as the Chinese population grew in a city, the number of Chinese associations multiplied and differentiated, with different criteria defining various associations, whether locality (provenance), clan (family name), or activities (sports, opera, etc.).⁶ My historical research indicated that Phnom Penh appeared to be an exception to this trend, in that a large Chinese population before independence supported only five associations, the *huiguan* representing the five speech groups: Teochew, Hokkien, Cantonese, Hainanese, and Hakka. This was a result of the French practice of indirect rule over the Chinese through their *huiguan*, called *congrégations* by the French, whose leaders (*chefs des*

congrégations), had immense power to suppress any rival associations that might be launched.⁷

After independence in 1953, Phnom Penh rapidly became more like similar-sized Chinese communities elsewhere, with a plethora of different kinds of associations. By the time of my research, clan associations (*tongxinghui*) had emerged among the Teochew, locality associations (*tongxianghui*) among the Cantonese, thirty-one sports clubs, opera groups, a mutual aid society, and fifty Chinese schools (two hundred in the whole country).

Bill Skinner's study of leadership in the Chinese community of Bangkok was my model, although I was able to produce only a shadow replica of that fine study; indeed, my own efforts increased my admiration for his sterling work.⁸ I discovered a complicated structure of associations at the apex of which stood the board of the Chinese hospital, comprising representatives of all the major associations. I observed the operation of this structure twice during my fieldwork, once in organizing the great God Procession (*You shen*) at Yuanxiao fifteen days after Chinese New Year, which is described below. The other event was the state visit in May 1963 of the Chinese president Liu Shaoqi. For security reasons, the Chinese were given only five days to organize his welcome and only a day's notice of his arrival. The hospital board established an organizing committee, which delegated responsibilities to subcommittees to assure that the president was met by waving crowds of schoolchildren and groups of adults, that nothing marred his stay, and that he was suitably entertained by the Chinese community. I concluded from these events that there was an active and effective associational structure in the community despite its diverse elements.

In my fieldwork, I defined as Chinese all those who participated in the Chinese associations available to them. I argued that this was methodologically sound because much of my count of Chinese was based on interviews with the leaders of associations, who would have an idea of their membership numbers. Obviously, it would have been impossible to ask every individual for his or her ethnic identity. I argued, too, that it was also theoretically defensible, because neither nationality nor language could be used as an indicator of Chinese identity, as many families included individuals of different nationalities and even different declared mother tongues. It was only much later in my academic life that I became interested theoretically in identity, really after I moved from Canada to New Zealand and my own identity was challenged—but that is a different story.⁹

In 1981, I published a paper declaring the end of the Chinese community in Cambodia due to the tragic changes Cambodia had suffered since my fieldwork there.¹⁰ The right-wing

coup in 1970 brought to power the anti-Chinese government of Lon Nol, and during his tenure Cambodia was carpet bombed by American B52's releasing thousand-pound bombs on the countryside, forcing much of the rural population into the cities as refugees.¹¹ One eyewitness said "the ground shook continuously" during the height of the bombing in 1972.

Lon Nol was followed by nearly five years of the disastrous regime of the Khmer Rouge, which declared "Year Zero" in 1975 and emptied the cities overnight. That, plus the deliberate elimination of the commercial sector, destroyed any remaining Chinese community. For me, the end of Chinese associations marked the end of Chinese identity, and therefore the end of any research possibilities on Chinese in Cambodia.

The evidence Dr. Penny Edwards presented to the Association for Asian Studies (AAS) in 1996 corroborated what I had written in 1981: at that time no Chinese community existed in Cambodia.¹² However, she reported that, since 1990, a vibrant Chinese community had reemerged after the State of Cambodia introduced a policy of cultural and religious freedom for Chinese and encouraged immigration again. The renaissance of the Chinese community following an eclipse of twenty years has attracted the fine young scholars who are presenting papers on the subject at this conference.

As a coda to my own research, I published a paper in 1990 on the annual God Procession at Yuanxiao, which was held annually until 1969, a spectacular event involving the whole Chinese community and most of the city as well.¹³ On two successive days, a procession of eighty possessed mediums from various temples wound through the central city, each colorfully dressed and sitting or standing on a sedan chair surrounded by a cortege of attendants, musicians, and dancers. Many of the mediums had skewers through their cheeks; others beat their backs with spiked balls; and some were sitting on knife blades or lying on beds of nails. As the bloody mediums arrived at the front gate of the Royal Palace, they got down from their chairs to dance among exploding firecrackers, cutting their tongues with a sword and licking the blood onto small pieces of yellow paper that were sold as powerful charms.

My research suggests that this event was unique in the world: while individual temples elsewhere sometimes paraded their idol and a possessed medium,¹⁴ the only reference I could find to a mass event like the God Procession was in Fuzhou (southeastern China), where it occurred at times of disasters such as drought or plague, not as an annual event.¹⁵

Those of you who have read my second book will recognize that in the final chapter it wanders off into China, returning to the interest that took me into the Chinese diaspora in the first place.¹⁶ And after Cambodia, that is where much of my research has focused. However, living in Cambodia for a year, I could not help but become interested in Cambodian politics generally, and especially in the policies of Prince Norodom Sihanouk and the Kampuchean Communist Party, both of which were subjects of papers I have published.¹⁷

Before I went to Cambodia, my image of Sihanouk came from the American media: a playboy prince who made bad films, a womanizer with a high-pitched voice. My first sight of him was at a volleyball match in front of the Royal Palace. We were standing at the court awaiting his arrival, when suddenly all the Khmer around me went down on one knee and held their hands in front of their faces, palms together. I turned around to see him coming through the trees in his shorts and T-shirt surrounded by attendants and six Alsatian dogs, the former and future God-King of Kampuchea. I saw him next as host of the Water Festival, an annual event on the waterfront when the king orders the Tonlé Sap River to change direction—and it has obeyed him every time since the fifteenth century!

My admiration for the prince was for his amazing prowess in keeping Cambodia out of two consecutive wars in Vietnam over twenty-five years and preserving Cambodian neutrality in the Cold War, despite strong forces within his own country trying to shift him to the right or to the left. Sometimes his politics appeared inconsistent as he moved slightly one way or the other to disarm his opposition from either side, but it was a remarkable and courageous feat.

After I left Cambodia, I met Prince Sihanouk twice, both times in Beijing. In November 1971, I spent an hour alone with him for a wide-ranging discussion in French. If I had been a journalist, I would have had a brilliant scoop, for he told me then that Ieng Sary had recently come to Beijing to request that he visit the Khmer Rouge government in the maquis, and he had agreed to do so, a promise he kept the following year. He invited me to attend the Cambodian National Day banquet presided over by Premier Zhou Enlai a few days later.

Then I met him again in 1980, when he was living in the former French legation (an appropriate residence, I thought!), and we talked in English. He was hoping then to return permanently to Phnom Penh, as he eventually did in November 1991.

My interest in the Kampuchean Communist Party was sparked by an intellectual puzzle, and it provided me with a moment of what I can only describe as intellectual ecstasy

when I solved it. For some time, I had been pro–Khmer Rouge, as were many scholars on the left (including Malcolm Caldwell) who subscribed to the *Bulletin of Concerned Asian Scholars*, and I was pleased when Prince Sihanouk joined forces with them to form the Government Royal de l’Union Nationale du Kampuchea. But I was utterly puzzled by the KCP’s analysis of Cambodian society.

A KCP report by Pol Pot stated that 85 percent of the Cambodian population were peasants,¹⁸ a fact that no one could dispute (my own research concluded that 86 percent of the population was peasantry),¹⁹ so the revolution would have to be based on the peasantry. He went on to say that their “scientific analysis,” supported by “direct investigation over several years,” concluded that the “fundamental contradiction” in Khmer society was the antagonistic relationship between landlord and peasant. Presumably this conclusion was based on the doctoral dissertation of Khieu Samphan at the University of Paris²⁰ (supplemented by the dissertations of Hou Yuon in Paris and Hu Nim in Phnom Penh),²¹ complemented by the five years the Khmer Rouge spent among peasants in the maquis. It indicated to the KCP that revolutionary land reform would be popular among the vast majority of the Khmer people.

This analysis of Khmer society was contradicted by another study I had recently read: *Le Paysan Cambodgien* by Jean Delvert, a mild-mannered French geographer whom I met in 1962 when he had just published his ethnography.²² His study was based on meticulously detailed fieldwork among the peasantry throughout Cambodia. Delvert’s conclusion was exactly the opposite of Pol Pot’s: “Cambodia knows no agrarian problem,” he wrote. Delvert’s research showed that there were very few tenant farmers and fewer landlords, that the large majority were peasant proprietors:

With important nuances which contribute to regional differences in livelihood (large properties in Battambang, Prey Veng and Svayrieng...) Cambodian peasant society is a democracy of small owner-tillers, under the commercial domination of the Chinese and at a mediocre standard of living. (1961, 219)

Delvert’s analysis showed that there was very little exploitation by landlords—but considerable exploitation by Chinese. In fact, the rural Chinese merchants, of whom there were many in Cambodia, exploited the Khmer peasants in three ways: they sold urban goods to the peasants at a profit, they bought the peasants’ rice at a price set by the merchant, and they provided usurious loans to tide the peasants over from one harvest to the next. The crucial fact, however, was that the Chinese merchants were not part of Khmer society: they

were outside it, and were therefore not seen as exploiters but rather as providing services to the peasants, just as the monsoon did. Ethnic difference, ignored by most Marxists, was crucial to the peasants' perception of exploitation.

So my puzzle was, how could the KCP have gotten it so wrong? The answer came to me in a "Eureka!" moment of intellectual ecstasy at 2:00 a.m. one night in 1979 when I was a visiting fellow at the University of Victoria, British Columbia. Looking at Khieu Samphan's tables of peasant classes in preparation for a lecture, I suddenly realized that they were based on figures from Battambang, Prey Veng, and Svay Rieng, the very provinces that Delvert had singled out as exceptions in his analysis. French agrarian policies in these three provinces had produced large properties with tenant farmers, in complete contrast to the majority of Cambodia's rice lands. Furthermore, when the Khmer Rouge went into the maquis following the Samlaut Revolt in 1967, their main base was in Battambang Province, so their rural experience for five years was in the landlord-exploited region of the country. Khieu Samphan's analysis, reinforced so congruently by their own experience in the countryside, led the KCP to misread their own society and develop a disastrous rural policy, so unpopular that it contributed to the horrendous repression the Cambodian people suffered in "Democratic" Kampuchea for four long years.

I published this analysis in an article entitled "Analytical Errors of the Kampuchean Communist Party" in 1981.²³ It wasn't a very sexy title, but I believe it was the best thing I ever wrote.

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Notes

1. Baker (1968).
2. Wickberg (1982).
3. D. Willmott (1960).
4. Skinner (1964).
5. Williams (1967, 173).
6. Freedman (1960, 25-48).
7. W. E. Willmott (1969, 282-301).
8. Skinner (1958).
9. See, for example, W. E. Willmott (1989).
10. W. E. Willmott (1981).
11. Shawcross (1979). See also Shawcross (1977).
12. Edwards (1996).
13. W. E. Willmott (1990).

14. See, for example, Elliott (1955).
15. Doolittle (1866).
16. The last chapter of W. E. Willmott (1970) speculates on what this overseas community structure can tell us about the nature of traditional urban social organization in China.
17. See, for examples, W. E. Willmott (1967a) and note 21, below.
18. Pot (1977, 20) (my translation).
19. See table XV in W. E. Willmott (1967b, 63).
20. Samphan (1979).
21. See Hou (1955) and Hu (1965). Hu's dissertation makes more explicit the derivation of Khieu Samphan's analysis from that of Mao Zedong.
22. Delvert (1961).
23. W. E. Willmott (1981a).

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